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Employment effects of vaccine mandate defiance

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Richard Fabling

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Author contact details

Richard Fabling
Senior Research Associate, Motu Research
richard.fabling@xtra.co.nz

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Disclaimer

These results are not official statistics. They have been created for research purposes from the Integrated Data Infrastructure (IDI) which is carefully managed by Stats NZ. For more information about the IDI please visit https://www.stats.govt.nz/integrated-data/. The results are based in part on tax data supplied by Inland Revenue to Stats NZ under the Tax Administration Act 1994 for statistical purposes. Any discussion of data limitations or weaknesses is in the context of using the IDI for statistical purposes, and is not related to the data's ability to support Inland Revenue's core operational requirements. Access to the data used in this study was provided by Stats NZ under conditions designed to give effect to the security and confidentiality provisions of the Data and Statistics Act 2022. The results presented in this study are the work of the author, not Stats NZ or individual data suppliers.

Motu Economic and Public Policy Research

PO Box 24390 info@motu.org.nz +64 4 9394250
Wellington www.motu.org.nz
New Zealand

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Abstract

We estimate employment effects of Covid-19 vaccine mandates on workers in the New Zealand education and health sectors. Unvaccinated workers are classified based on whether they comply with or defy the mandate. We identify substantial heterogeneity in outcomes across these two groups, using already-vaccinated workers as the control group in a difference-in-difference regression framework. Mandate defiers experience ``permanent'' labour market scarring with higher non-employment, lower wages conditional on employment, and increased mobility within New Zealand, consistent with moving to work opportunities. These effects weaken over time but persist for at least two and a half years.

JEL codes

I18 – government policy, regulation, public health; J63 – labour turnover; vacancies; layoffs

Keywords

Covid-19; vaccine mandates; workers; job displacement

Summary haiku

Thou shalt vaccinate

came the command from above.

Defiers suffer

1 Motivation

The New Zealand Government imposed vaccine mandates on workers in multiple industries as part of its response to the Covid-19 pandemic. The stated legislative aim of these mandates was “to prevent, and limit the risk of, the outbreak or spread of COVID-19 by requiring certain work to be carried out by affected persons who are vaccinated.”[1]

We study the labour market effect of these mandates in the education and health sectors – the largest affected industries, together employing over 370,000 workers. We focus on skilled workers with a degree qualification and who are likely to have substantial sector-specific human capital at stake. These workers were predominantly vaccinated prior to mandating, particularly in the health sector which had priority access to vaccines (Fabling 2026). We exploit this fact to select a natural control group for affected workers, namely same-sector workers vaccinated (at least once) prior to the mandate announcement on 11 October 2021.

The mandate required initial Covid-19 vaccination by 15 November 2021 to enable full vaccination (ie, two doses) by 1 January 2022.[2] Workers defying the order become legally unable to perform their role in the workplace, potentially causing employment loss. The unvaccinated are separated into two groups that either comply (compliers) or do not comply (defiers) with the mandate by receiving a first vaccination by the November deadline. This classification is mechanical and we validate it using additional vaccination behaviour – considering Covid-19 vaccination rates after the mandates expire, and influenza vaccination rates before and after the Covid-19 pandemic.

Our analysis doesn’t critique the decision to impose mandates or assess the health benefits of them. Rather, we focus on quantifying the costs on workers who were unvaccinated and, therefore, had to decide whether to comply or not. As a by-product of this analysis, we estimate the effect of mandates on sector vaccination rates. In addition to comparing complier and defier workers’ employment patterns to the control group, we also look at wage and migration effects to draw a broader picture of the effect of mandates on skilled workers.

Section 2 outlines the data and methods that we use. Section 3 reports estimates of the impact of vaccine mandates on defiers, while section 4 summarises findings.[3]

2 Data & method

2.1 Data

We use New Zealand’s Integrated Data Infrastructure (IDI) and related user-generated tables to estimate the impact of vaccine mandate defiance on worker outcomes. Fabling-Maré’s labour

[1]Quoted from clause 3 of the COVID-19 Public Health Response (Vaccinations) Order 2021.
[2]Mandate deadlines come from DPMC (2023) and New Zealand Government (2021a, 2021b).
[3]We do not compare results with previous findings for New Zealand for reasons discussed in the appendix.

tables identify employer-employee relationships, monthly earnings, job spells and employment histories (Fabling and Maré 2015; 2020). These data cover our full population of interest – workers employed in the education or health sector in the month prior to the mandate announcement (September 2021, the base month) – and allow us to track these workers to jobs outside of these sectors.

Table 1 shows how restrictions affect the analysis population.[4] The primary restriction we make is to exclude workers who do not have a degree, which removes 32% of education workers and 43% of health workers. Consistent with our initial motivation to focus on workers with sector-specific skills, Census testing suggests that this restriction increases the proportion of workers whose occupations are aligned with the sector (eg, teachers and doctors, rather than administrative staff). While the mandate covered many workers in unaligned occupations – eg, customer-facing support roles – the "skilled" population is more homogeneous, which is useful for comparing the control and affected groups since we cannot control directly for current occupation.

Other restrictions are relatively minor, with the main additional restriction being the exclusion of workers whose in-scope job is a secondary source of wage and salary income (1.2-1.5% of the initial population). We also remove workers with unusually low or unusually high monthly earnings (around 0.5% of workers).[5] While the age of eligibility for national superannuation in New Zealand is 65, we include workers aged 20-74 year olds because the post-retirement age education workforce is non-trivial.

Missing characteristics affect only a small proportion of workers, due to the use of multiple administrative and survey sources in the IDI, as well as limited use of imputed values. For our analysis, we require sex, ethnicity, country of birth and cleaned residential meshblock location mapped to Stats NZ's Functional Urban Area (FUA).[6]

Day of Covid-19 and influenza vaccination data are provided by the Ministry of Health and we use the latter to identify workers who have a history of vaccination. We require each worker to be linked to the Ministry of Health dataset in the IDI. This restriction does not require workers to have been vaccinated but, rather, means that any Covid-19 data an individual has is linked to their employment history and other data in the IDI. Population loses from this restriction are around 0.4% for each sector. After all restrictions, the analysis population is 100,218 education workers and 118,110 health workers.

[4] The table note lists the in-scope industry codes by sector.

[5] For education (health), this excludes workers with earnings below $200 ($300) or above $14K ($42K) in the base month.

[6] Highest qualification and country of birth are included in a new employee and working proprietor table on IDI_Adhoc (see Fabling 2026, Appendix A). Fabling and Maré (2020) describes the methodology for meshblock locations. Labour tables are updated annually by the author, and available to all authorised users of the IDI IR schema via secure Datalab access. Birth month and level one ethnicity data come from Stats NZ's personal details table on the IDI. We use the 202410 IDI instance, which is the latest available at the time of analysis.

2.2 Method

To identify the effect of mandates on affected workers in the mandated sectors, we use a regression approach equivalent to difference-in-difference (DiD) with covariates. Affected workers are mechanically identified based on the timing of the mandate announcement and associated deadlines.[7] The already-vaccinated at the time of the announcement are the DiD *control* group, while affected workers are classified as either *compliers* – who satisfy the first vaccination deadline, or *defiers* – who do not.

Figures 1 and 2 report vaccination rates for these three worker groups for initial vaccination, second vaccination (fully vaccinated) and first booster. By construction, the initial vaccination rate for the control group is 100% by 10 October (solid line, panel A). Compliers (dashed line) all receive their first vaccination between 11 October and 15 November, which is the last day it is feasible to be fully vaccinated by 31 December 2021 (six weeks later). Defiers (dotted line) do not satisfy the 15 November deadline, but may become vaccinated subsequently, which is the case for around 20% of all defiers.

Workers who receive an initial Covid-19 vaccination are highly likely to become fully vaccinated – with vaccination rates close to 100% for control and complier groups, and over 90% for defiers (figures 1B & 2B). For this reason, we focus entirely on initial vaccination to classify workers, ignoring the fact that a very small proportion of compliers have not fully complied (are not fully vaccinated). Because the subpopulation is non-trivial in size, we perform additional exploratory analysis of defiers who become fully vaccinated by 1 February 2022 ("defier-compliers") – in time for the start of the 2022 school year – to assess whether their employment outcomes differ from other defiers.

The main cross-sector differences in vaccination patterns stems from the prioritisation of health workers in vaccine access, which lead to earlier first (and, consequently, second) vaccination in that sector compared to the education sector, and higher overall vaccination rates in the health sector prior to mandating. The first column of table 2 reports group sizes in each sector, indicating that the already-vaccinated (control) constitute 94% of health sector workers, compared to 88% of education sector workers.[8] While some of this difference may be due to prioritisation, all workers were able to receive an initial vaccine dose from 1 September and had, therefore, had at least five weeks of access prior to the mandate announcement.

In principle, workers labelled as (affected) compliers could always have intended to be vaccinated. Two facts push back against this logic. Firstly, in the education sector, booster shots for the fully vaccinated were not mandated. Thus, figure 1C reveals vaccination preferences in the absence of a mandate. Here we see that 90% of fully vaccinated control group workers receive a booster, but only 25% of compliers do and almost no defier-compliers do. Compliers, therefore, appear to have a strong preference to not be boosted if they do not have to be.

[7] Meehan et al. (2024) determine that mandates were probably unanticipated for these workers.

[8] Fabling (2026) compares vaccination rates over time for all industries, showing that the education sector was relatively fast to become vaccinated compared to most private-sector-dominated industries.

The equivalent pattern for health sector compliers and defiers is weaker (figure 2C), probably because the health mandate was extended to require a booster dose within six months of full vaccination. Despite the mandate extension, revealed preferences for compliers and defiers are still materially different from already-vaccinated (control) workers.

The second piece of evidence comes from worker influenza vaccination patterns in the years prior to the Covid-19 pandemic (table 2). Fabling (2026) demonstrates that prior influenza vaccination is a strong predictor of early Covid-19 vaccine uptake. That pattern is replicated here in the ratio of influenza vaccination rates across groups. Prior to the pandemic, control group workers were three to five times more likely to be vaccinated for influenza than defier workers, and two to three times more likely to be vaccinated than complier workers. This comparison points to general differences in attitudes toward vaccination across groups and/or differences in at-risk characteristics for respiratory infection.

Post-pandemic patterns of influenza vaccination are also interesting, with a decline in vaccination rates for defiers and an increase in vaccination rates for control group workers. Overall, therefore, the gap in influenza vaccination rates increases for both compliers and defiers.

Another potential criticism of the worker classification is that defiers do not become vaccinated because they are outside the scope of the mandate – ie, they are not "defying" anything. The first argument against this view has already been made – that the mandate was broad, including all workers who might have contact with patients/residents/students. We can also test this hypothesis by looking at the subset of workers who have a Census occupation response prior to the pandemic (ie, from 2013 or 2018). The bottom two rows of table 3 report the coverage rate of these Census data and the proportion of covered workers whose occupation is ever a sector-specific match. While defiers are 4-5pp less likely to be in a matching occupation, relative to the control group, these differences are too small to explain a significant proportion of non-compliance with mandating.

All tests, therefore, are consistent with the complier and defier groups being subject to mandating and, in the case of compliers, largely being vaccinated *because* the mandate required it. This latter distinction matters for conclusions on the impact of mandates on overall sector vaccination rates.

With all workers assigned to the control, complier (indicator variable, δ_i^C) or defier (δ_i^D) group, we estimate DiD equations for each sector of the form:

$$Y_{it} = \alpha_t + \beta_t^C \delta_i^C + \beta_t^D \delta_i^D + \beta_t^Z Z_i + \epsilon_{it} \quad (1)$$

with corresponding time-varying coefficients of interest β_t^C and β_t^D capturing the mean difference between each affected group and the already-vaccinated (control). ϵ_{it} is an error term and Z_i is additional covariates that are correlated with the outcome of interest (Y_{it}) and with vaccination status.[9] We estimate separately for each t, since coefficients are month-specific.

[9] Covariates are base month (Sept-21) values, including outcomes where these differ across workers. The inclusion of base period outcome values in Z_i makes our approach equivalent to DiD with covariates.

We focus on three main outcomes – within-sector employment; monthly (log) wages; and residential location – reporting estimated monthly coefficients graphically over February 2020 to July 2024. All three outcomes are conditional on a worker still being alive and in New Zealand, and wages effects are further restricted to workers in employment. For key months, we produce estimated coefficients for three additional outcomes – remaining within New Zealand; any employment; and monthly (log) wages conditional on having left the sector.

Means of covariates (Z_i) are summarised in table 3. Both the education and health sectors are female-dominated industries, with women less likely to be already-vaccinated (in the control group) than men. Compliers are younger than control group workers, especially in the education sector, and, therefore have less within-sector employment experience. Workers with post-graduate degrees are more likely to be vaccinated early, consistent with Fabling (2026) who finds a strong positive gradient between formal education levels and vaccine uptake in the New Zealand workforce.

For the education sector, early vaccination is less common in early childhood education compared to primary and secondary education. In the health sector, early vaccination is most common in hospital workers. Only a small proportion of workers (3-4%) have a secondary job outside the sector. Differences in employer private-for-profit status reflect the industry composition of jobs, with private sector jobs more prevalent in early childhood education and in residential care facilities.

Mean monthly wage differences capture group differences in gender, experience, and qualification as well as pay gaps across sub-industries related to public sector funding models. In the education sector, mean complier/defier wages are 10-11% below control group wages. In the health sector the equivalent gap is 16-18%. Māori and Pacific ethnicity workers are less likely to be vaccinated early, while Asian ethnicity workers are more likely to be vaccinated early. Finally, workers living in the Auckland Functional Urban Area (FUA) are more likely to be vaccinated early than other regions in New Zealand, particularly the rest of the North Island.

3 Results

3.1 Raw outcome comparison across groups

Figures 3 and 4 plot raw employment rates across worker groups – already-vaccinated (control group, solid line), compliers (dashed line) and defiers (dotted line). In panel (A) the outcome is within-sector employment, whereas panel (B) shows the employment rate, regardless of sector. These figures do not account for key differences across groups, such as age, which are correlated with labour market participation and job churn (table 3).

Focussing first on the education sector (figure 3), we observe seasonal (January) dips in employment across all groups, coinciding with the extended summer break between school years. Both before and after mandating, the seasonal decline is stronger for defiers, suggesting that this group has a higher proportion of substitute (on-call) teachers, than other education worker groups.

By construction, all workers have within-sector employment in the base month – September 2021 – and this employment rate decays both before and after that month reflecting exit from employment and cross-sector job moves. Raw employment rates in panel (B) capture jobs in all sectors and, therefore, are higher than in panel (A). Comparison of control worker outcomes in the two panels in figure 3 implies that the main driver of attrition from the education sector over this period is transitions out of employment. Specifically, the within-sector employment rate in July 2024 is 82% for already-vaccinated workers, while the employment rate across all jobs is 89%. Therefore, 7% (89−82) of the control group have left the sector for another job, while 11% (100−89) have no job.

Compliers have higher attrition from the education sector (and employment more generally) than control group workers both before and after the base month, which suggests that the employment impact of being mandated and complying is low, and that differences in other worker characteristics likely explain differing exit rates. Defiers on the other hand experience a substantial drop in education sector employment following the mandate with 61% of workers no longer employed in the sector in March 2022. This negative impact is offset somewhat by employment in other sectors with 51% of defier workers not employed at all (ie, a 10pp higher employment rate). The employment gap for defiers recovers over time, but persists into the 2024 school year with the within-sector (any) employment rate at 64% (75%) in July 2024.

For the health sector (figure 4), there is clear asymmetry in attrition rates before and after the base month for both compliers and defiers. The post-mandate gap in within-sector employment rate (panel A) between compliers and the control group grows to 11pp by April 2022 and remains at a similar level. In contrast, the gap in overall employment reaches a similar peak but then closes over time. For defiers, the within-sector (any) employment rate falls to 43% (57%) in May 2022. As in the education sector, the defier employment rate gap recovers somewhat but not fully by the final analysis month.

Despite the sizeable initial drop in defier employment in each sector, we should question why that drop is not much larger. We have already addressed one reason, which is that some defier jobs are not covered by the mandate, which we discounted on the basis of Census occupation comparisons and the broad coverage of the mandate. An alternative explanation is employer non-compliance with enforcement, however this would suggest observable differences between defier employment rates in public and private institutions. Such differences are not apparent in the data.

A third possibility is that defiers who subsequently become vaccinated ("defier-compliers") explain ongoing within-sector employment, particularly in education where the start of the school year gave some additional leeway for defying workers to be vaccinated before students returned. However, the defier-complier group constitutes less than 20% of the defier group (figures 1 & 2) so this group cannot explain the majority of the remaining employment among defiers. Figure 5 demonstrates this by separating defier-compliers (light dashed line) from ongoing defiers (solid line) for within-sector education (panel A) and health (panel B) employment. For reference, complier (dark dashed) and defier (dotted) employment rates are repeated from earlier figures. In both sectors, defier-compliers are more similar to compliers – in terms of employment outcomes – than they are to defiers, though they still experience worse employment rates than compliers. The mean employment rate for defiers, therefore, is bolstered by defier-compliers retaining sector jobs. Without this subgroup, the employment rate for defiers drops to a low of 31% (in March 2022) in the education sector, and a low of 38% (in June 2022) for the health sector.

Setting aside these critiques, it seems most probable that legal workarounds allowed some defiers to continue to be employed. In particular, the mandates prevented on-site work, but did not preclude working from home if that was feasible. Such work could result in reduced hours – with potential implications for earnings – if aspects of the role can only be completed on-site.[10]

3.2 Estimated mandate effects

Figures 6-11 plot estimated mandate effects as the difference between outcomes for affected groups and the already-vaccinated (control) group, with additional covariates controlling for potential confounding relationship (eg, the fact that younger workers are more geographically mobile and less likely to be vaccinated early). Relevant coefficients (β_t^C, β_t^D from equation 1) are estimated by month and sector (education in panel A and health in panel B) with 95% confidence intervals included. By construction, differences are zero in the base month (September 2021) and this month is excluded from estimation.

Across all figures, the addition of covariates tends to remove the pre-mandate trend differences in outcomes that we observed in the raw statistics. Columns one and four of table 4 report coefficients and standard errors for the first analysis month as an indicator of potential parallel trends violations. Focussing on the three outcomes presented graphically, log wages are significantly lower for education sector compliers (1pp) and this is consistently true over the pre-mandate period (figure 7A).[11] Other initial differences for compliers are not different from zero (at the 95% level), except for health sector location, which is 1pp more likely to be stable for compliers (than control group workers).

For defiers, all three outcome gaps are negative in February 2020, though not statistically significant at the 95% level for within-sector health worker employment. Aside from residential location, these pre-mandate gaps are stable over time (figures 9-11). Standard errors associated with FUA coefficients are relatively large, but point estimates suggest slightly higher in-

[10]Because the employer-employee data does not include hours worked information for most workers, the way employee FTE is estimated will result in an apparent drop in the monthly (log) wage in many cases (Fabling and Maré 2015).

[11]Wage gaps in log points (reported in figures and tables) are converted to percentage points in the text for consistency with other outcomes ($pp = e^{lp} - 1$).

ternal migration rates pre-mandate after controlling for covariates – in the range of 1-2pp over those 18 months. Together, these results suggest that the selected control group and covariates adequately address concerns about underlying trend differences causing estimated mandate effects to be biased.

For education sector compliers (figures 6A-8A) the effect of mandates on each of the three outcomes are negative, statistically significant and persist until the final analysis month. Columns two and three of table 4 (top panel) report βs at March 2022 and July 2024. Within-sector job loss is initially around 3pp before recovering to around 1pp. Log wages are initially similar to pre-mandate levels, but are 2pp below expectation by the final month, while education sector compliers are 1pp more likely to be living in a different urban area in that month. These gaps, while statistically significant, are economically small and – in the case of wages and location – develop slowly, rather than immediately after the mandate.

Mandate employment and wage effects are stronger for health sector compliers (figures 6B-8B). Within-sector employment falls by almost 9pp by March 2022, before recovering to 5.5pp below expectation. Vaccine mandate requirements were removed from the health sector on 26 September 2022, which aligns with the beginning of within-sector employment recovery. Despite the recovery in employment, wages remain 4-5pp below expectation from March 2022 until the end of the analysis period.

The stronger initial impact on health workers may reflect the fact that the original mandate announcement imposed a first vaccination deadline of 31 October in the health sector. This decision was altered on 23 October to require health sector first vaccinations by 15 November, matching the education sector deadline (New Zealand Government 2021a, 2021b). The short window of time between announcements limits the potential for health workers to make and implement exit decisions based on the earlier deadline. Furthermore, these workers were all vaccinated by 15 November suggesting that they are not defiers misidentified as compliers because of the deadline change.

An alternative rationale for differences in complier employment effects across sectors is compliers who regret their decision to conform and leave the sector despite vaccinating to meet mandate requirements. If these “regretters” are more prevalent in health compared to education then this would cause a gap in employment effects. Health sector workers were targeted for early vaccination and are more vaccinated prior to the mandate, which opens the possibility that unvaccinated health workers have more strongly self-selected into non-vaccination than unvaccinated education workers.

This thesis is supported by the relative size of the defier and complier groups across the two sectors (table 2). While the defier group is a higher proportion of overall workers in the education sector (4.2%) than in the health sector (2.8%), this ranking reflects the higher share of already-vaccinated health workers. As a proportion of the unvaccinated (ie, defiers plus compliers), defiers constitute 48% for the health sector and 36% for the education sector. It is possible, therefore, that compliers in the health sector are closer to the margin of defying than they are in the education sector and, with time, regret their decision enough to leave the sector.

Table 4 includes results for three further outcomes – any employment, wages conditional on leaving the sector, and remaining within New Zealand. Focussing on estimates for July 2024, job opportunities outside the relevant sector do not completely compensate for within-sector job loss. Further, the supplemental wage results suggest that workers who leave the sector but are still employed take substantially lower wages in that transition, which may be due to loss of wage premia associated with sector-specific skills and/or reflect the difficulty of finding a suitable alternative job match during the pandemic. Finally, the results show that compliers are around 1pp more likely to remain in New Zealand than the already-vaccinated (control) group.

Turning to the impact on defiers over time (figures 9-11), the results show sudden loss of employment, a substantial wage decline and increased internal migration consistent with moving to find work. For education sector defiers, within-sector job loss is 52pp at March 2022 mitigated somewhat when all jobs are considered (44pp, table 4). The within-sector (any) job gap shrinks over time, but is still a substantial 16pp (12pp) in July 2024. Log wages initially rise, which is due to exit payments (including accrued leave), before ending at 7pp lower than expectation in part due to lower wages for workers who are no longer employed in the sector (13pp). While defiers are slightly more mobile than control group workers pre-mandate, they are even more likely to have moved region after defying the mandate with the timing of these moves consistent with being a causal effect of job loss. According to table 4, they are also more likely to have left New Zealand though the estimated effect is economically small (0.6pp).

For health sector workers, within-sector (any) job loss is 45pp (38pp) in March 2022, recovering to 24pp (16pp) by July 2024. Exit payouts are more substantial than in the education sector in the immediate aftermath of job loss, and “permanent” wage loss is also stronger at 11pp in July 2024, reflecting a larger penalty (17pp) on wages for workers no longer in health sector jobs, combined with a lower likelihood of returning to the sector than is the case for education workers. Post-mandate internal migration is evident for defiers in the health sector, but the effect (3pp at July 2024) is weaker than is the case of education workers. Point estimates suggest health sector defiers were more likely to leave New Zealand at a similar rate to education defiers, but the effect is not consistently statistically significant (at the 5% level).

4 Conclusions

The introduction of vaccine mandates in the education and health sectors appears to have raised vaccination rates among skilled workers who would have preferred to remain unvaccinated. We reach this conclusion based on the timing of worker vaccination and by examining other “willingness-to-vaccinate” evidence. In the education sector, 64% of the unvaccinated receive a first vaccine prior to the mandate deadline, raising the sector vaccination rate from 88% to 96%. In the health sector, 52% of the unvaccinated met the mandate requirement, raising the sector vaccination rate from 94% to 97%. At this simplest level, the mandate had success – workers became vaccinated allowing them to remain in their job. Beyond that, though, we do not assess whether the goal of reducing the risk of Covid-19 outbreaks was materially impacted by enforcing vaccination for in-person workers in these sectors. Nor do we assess additional potential benefits – for example, improved educational outcomes because caregivers had more confidence to send students back to school.

Regardless of the successes of the policy, we document substantial labour market costs for workers who defy the mandate. For defiers, there is clear labour market scarring driven by job loss in their pre-mandate sector, only partially compensated for by new jobs elsewhere. Using a back-of-the-envelope calculation, our estimates imply that 480 education and 500 health workers remain without jobs in July 2024 because of the mandates imposed in October 2021.[12] Displaced workers who find jobs outside the sector experience substantial "permanent" wage cuts, likely reflecting lost returns from sector-specific skills. The loss of these workers should be of concern, given both education and health sectors struggle to recruit and retain skilled workers.

Raw results for defiers are similar to displaced worker findings in Maré et al. (2023), who produce estimates for graduate workers in New Zealand (their figure A4). In their event study, they estimate an initial loss of employment for around 45% of skilled workers following firm closure. Over the long-run – four years post-closure in their analysis – non-employment continues to affect around 30% of skilled workers. Interestingly, the recovery in employment rate from firm closure takes around six months, which is approximately twice as fast as the recovery time for defiers. It seems likely that the unique labour market conditions during the pandemic had a cooling effect on job search, particularly for workers who were unvaccinated. In terms of wages (conditional on employment), Maré et al. (2023) find that negative effects on skilled workers fully dissipate after two and a half years, which is at odds with our finding of a "permanent" reduction in wages. A key difference between the two employment shocks is that displaced workers in the event study are free to make within-sector job moves, where this is not possible for defiers.

For compliers, the labour market effects are smaller with a decline in sector employment in the short-run, and long-run difference in overall employment corresponding to 130 workers in each sector without jobs in July 2024. For many compliers, their primary mandate cost may be submitting to the vaccine, which they tend to avoid when given free choice in the future.

This cost highlights concerns that mandates may affect future willingness to be vaccinated. Table 2 presents suggestive evidence of this effect, with the increasing bifurcation in the influenza vaccination rate across worker groups suggesting a bolstering effect of mandates on existing negative viewpoints. Fabling (2026) suggests employer-encouraged annual influenza vaccination as a potential route towards normalisation of vaccination.

[12] Estimated by taking the number of defiers still alive and in New Zealand in the last analysis month – 3,990 education & 3,051 health – and multiplying by the any job coefficient (-0.121 & -0.164, respectively).

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Tables

Table 1: Proportion of population lost to restrictions

	Education			Health		
	Workers	Loss		Workers	Loss	
	N	N	Proportion	N	N	Proportion
Sector population	**153,582**			**217,899**		
Not a graduate		48,531	0.316		94,317	0.433
Not main job		2,358	0.015		2,655	0.012
Earnings tail		984	0.006		1,191	0.005
Age not 20-74		690	0.004		336	0.002
Missing characteristics		168	0.001		513	0.002
No IDI health link		633	0.004		777	0.004
Analysis population	**100,218**		**0.347**	**118,110**		**0.458**

Initial population is all (EMS) wage and salary earners employed in an in-scope industry in September 2021 and present in New Zealand in that month. **Education**: Preschool Education (P8010); Primary Education (P8021); Secondary Education (P8022); Combined Primary and Secondary Education (P8023); Special School Education (P8024); and Child Care Services (Q8710). **Health**: Hospitals Except Psychiatric Hospitals (Q8401); Psychiatric Hospitals (Q8402); General Practice Medical Services (Q8511); Specialist Medical Services (Q8512); Pathology and Diagnostic Imaging Services (Q8520); Dental Services (Q8531); Optometry and Optical Dispensing (Q8532); Physiotherapy Services (Q8533); Chiropractic and Osteopathic Services (Q8534); Other Allied Health Services (Q8539); Ambulance Services (Q8591); Other Health Care Services (Q8599); Aged Care Residential Services (Q860); and Other Residential Care Services (Q8609).

Table 2: Influenza vaccination rate by group and calendar year 2018-2023

		Proportion vaccinated for influenza in calendar year					
	N(workers)	2018	2019	2020	2021	2022	2023
				Education			
Defier	4,182	0.030	0.039	0.057	0.026	0.011	0.013
Complier	7,443	0.049	0.054	0.104	0.061	0.075	0.069
Control	88,596	0.141	0.172	0.319	0.239	0.366	0.355
Ratio (Control/Defier)		4.7	4.4	5.6	9.3	34.0	27.5
Ratio (Control/Complier)		2.9	3.2	3.1	3.9	4.9	5.2
				Health			
Defier	3,291	0.043	0.046	0.087	0.046	0.026	0.028
Complier	3,543	0.071	0.081	0.159	0.113	0.112	0.120
Control	111,276	0.147	0.174	0.302	0.207	0.437	0.440
Ratio (Control/Defier)		3.4	3.7	3.5	4.5	16.5	15.6
Ratio (Control/Complier)		2.1	2.1	1.9	1.8	3.9	3.7

Control group is workers vaccinated prior to mandate announcement. Compliers follow mandate requirements and defiers do not follow those requirements.

Table 3: Mean worker characteristics at September 2021 by group and sector

	Education			Health		
	Control	Complier	Defier	Control	Complier	Defier
Female	0.806	0.838	0.857	0.784	0.837	0.851
Age (yrs)	45.1	38.6	44.4	42.1	39.6	44.6
Prior employment (yrs)	13.9	11.7	12.8	11.1	10.1	11.5
Within sector	10.8	8.2	9.5	8.2	6.3	7.9
Other	3.2	3.6	3.4	2.9	3.7	3.7
Post-graduate qualification	0.473	0.315	0.344	0.489	0.352	0.411
Early Childhood Education	0.242	0.389	0.317			
Primary & Secondary School	0.762	0.619	0.689			
Hospital				0.565	0.423	0.439
General Practice & Specialist				0.280	0.367	0.381
Residential Care				0.176	0.222	0.193
Secondary job outside sector	0.031	0.033	0.034	0.035	0.040	0.037
Private-for-profit employer	0.164	0.267	0.201	0.381	0.442	0.466
Monthly wage	$6,025	$5,412	$5,340	$7,319	$5,980	$6,123
European	0.796	0.721	0.795	0.605	0.609	0.711
Māori	0.123	0.277	0.194	0.083	0.174	0.136
Pacific	0.046	0.077	0.057	0.038	0.076	0.054
Asian	0.122	0.071	0.072	0.326	0.228	0.177
MELAA	0.013	0.014	0.014	0.023	0.038	0.026
NZ-born	0.689	0.782	0.714	0.483	0.570	0.576
Living in Auckland	0.341	0.197	0.250	0.331	0.209	0.248
Rest of North Island	0.432	0.564	0.543	0.427	0.519	0.495
South Island	0.227	0.238	0.207	0.243	0.272	0.257
Census coverage (13/18)	0.859	0.766	0.803	0.755	0.692	0.755
Occupation match	0.760	0.720	0.716	0.604	0.499	0.552

Prior employment left censored at 22 years. Industry shares within sectors do not sum to one because of multi-job holders. Monthly wage is geometric mean reflecting the use of log wage in analysis. Ethnicity shares do not sum to one because of multi-ethnicity workers. Other ethnicity grouped with European based on prior Census analysis. Occupation match is conditional on Census 2013 and/or 2018 response.

Table 4: **Summary of estimated effects at select months**

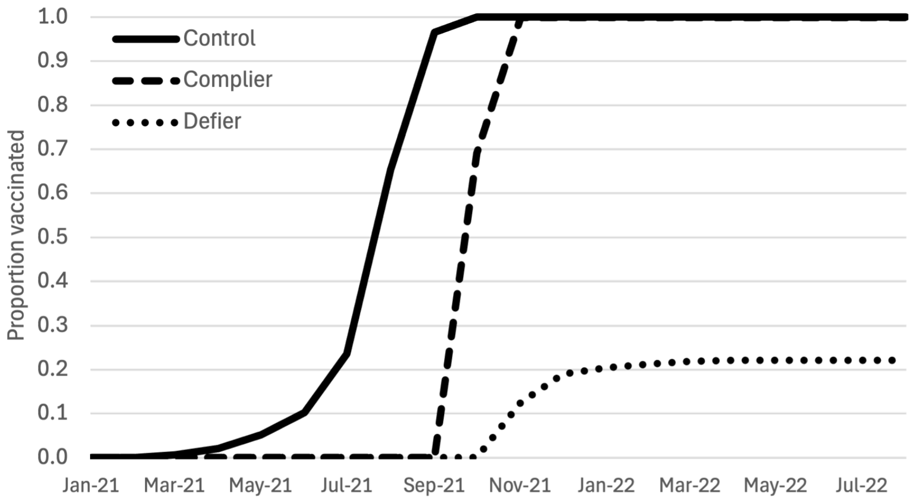
	Complier			Defier		
Education	Feb-20	Mar-22	Jul-24	Feb-20	Mar-22	Jul-24
Within sector job	-0.002	-0.029**	-0.012*	-0.032**	-0.518**	-0.164**
	[0.004]	[0.004]	[0.005]	[0.005]	[0.008]	[0.008]
Any job	-0.002	-0.034**	-0.018**	-0.027**	-0.443**	-0.121**
	[0.004]	[0.003]	[0.004]	[0.005]	[0.008]	[0.007]
Log wage	-0.012**	-0.013**	-0.020**	-0.014**	-0.029**	-0.071**
	[0.003]	[0.003]	[0.004]	[0.004]	[0.007]	[0.006]
Log wage (not in sector)	-0.019	-0.079*	-0.062**	-0.040	-0.169**	-0.136**
	[0.018]	[0.031]	[0.016]	[0.021]	[0.021]	[0.018]
Stable location (FUA)	-0.006	-0.003	-0.010*	-0.013**	-0.028**	-0.044**
	[0.004]	[0.003]	[0.004]	[0.005]	[0.004]	[0.006]
Within New Zealand	0.006**	0.003**	0.007**	0.003	-0.008**	-0.006*
	[0.001]	[0.001]	[0.002]	[0.002]	[0.002]	[0.003]
	Complier			Defier		
Health	Feb-20	Mar-22	Jul-24	Feb-20	Mar-22	Jul-24
Within sector job	-0.003	-0.088**	-0.055**	-0.011	-0.453**	-0.235**
	[0.005]	[0.006]	[0.008]	[0.006]	[0.009]	[0.009]
Any job	0.000	-0.081**	-0.038**	-0.014**	-0.379**	-0.164**
	[0.005]	[0.006]	[0.006]	[0.005]	[0.009]	[0.008]
Log wage	0.011	-0.041**	-0.049**	-0.016**	-0.072**	-0.112**
	[0.006]	[0.006]	[0.007]	[0.006]	[0.010]	[0.009]
Log wage (not in sector)	0.016	-0.103**	-0.075**	-0.040	-0.231**	-0.186**
	[0.022]	[0.032]	[0.022]	[0.021]	[0.023]	[0.018]
Stable location (FUA)	0.013*	0.000	0.004	-0.017**	-0.019**	-0.025**
	[0.005]	[0.004]	[0.006]	[0.006]	[0.004]	[0.006]
Within New Zealand	-0.002	0.004**	0.010**	0.001	-0.014**	-0.008
	[0.003]	[0.001]	[0.004]	[0.002]	[0.003]	[0.004]

Coefficients estimated using separate OLS regression for each outcome, month and sector (see equation 1). **;* indicates coefficient significantly different from zero at the 1%;5% level (robust standard errors in brackets). Selected months are first month (Feb-20); last month (Jul-24); and month of peak within sector employment effect for education sector defiers (Mar-22). The equivalent peak for health sector defiers occurs in May-22 (point estimate -0.460). Additional covariates in all regressions are base month: age by sex; employment history (years in sector; years outside sector; total years (0-22); sub-industry); log wage; ethnicity; post-graduate degree; residential location (functional urban area); country-of-birth; year arrived to live in NZ (for migrants).

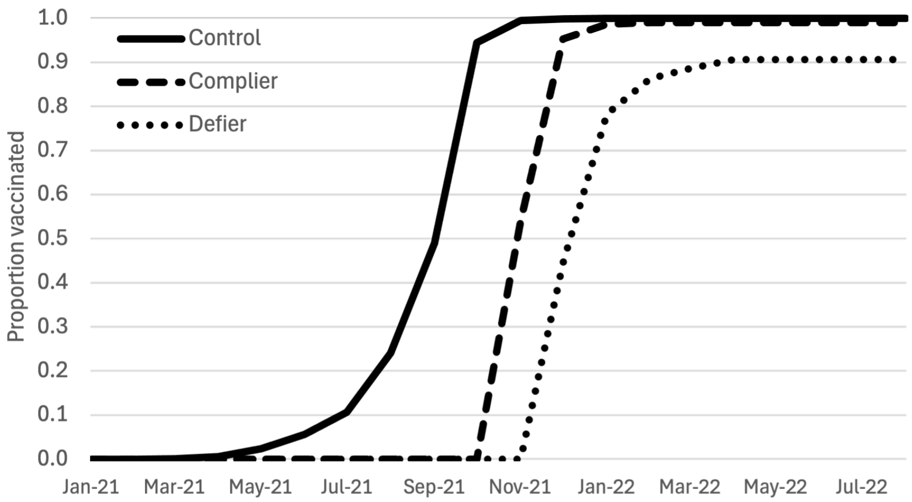
Figures

Figure 1: Covid-19 vaccination rate for education sector workers

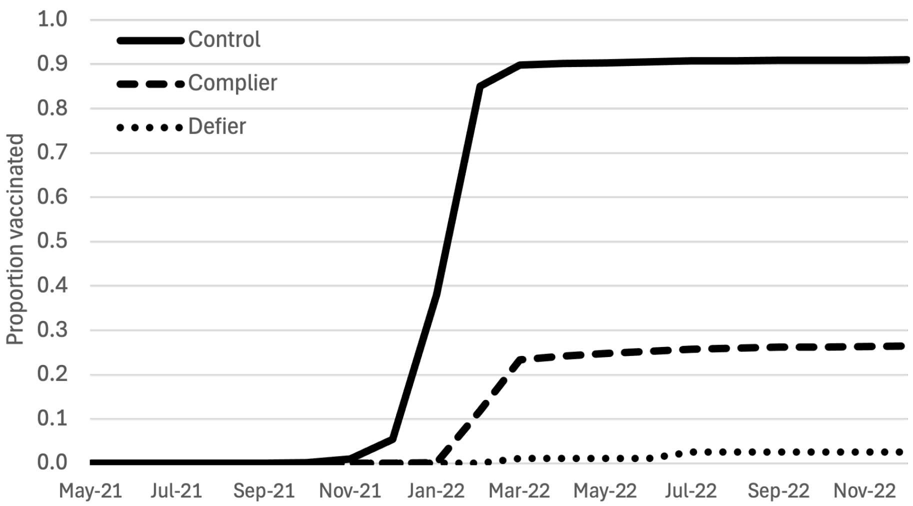
(A) Initial vaccination



(B) Second vaccination (fully vaccinated)

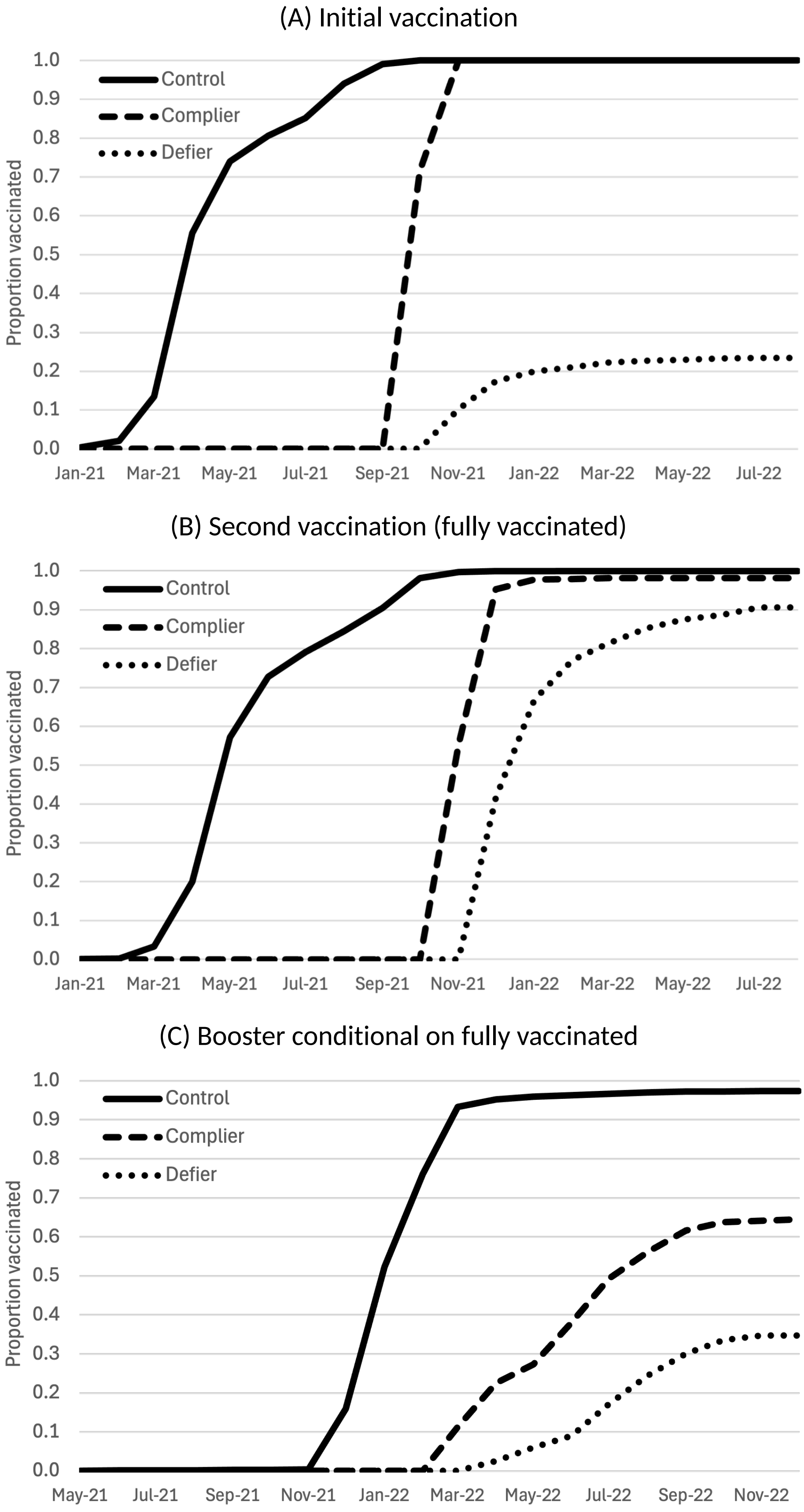


(C) Booster conditional on fully vaccinated



Panel (C) time period starts four months later than other panels. Vaccination rates as proportion of analysis population (ie, no adjustment for outward migration or death).

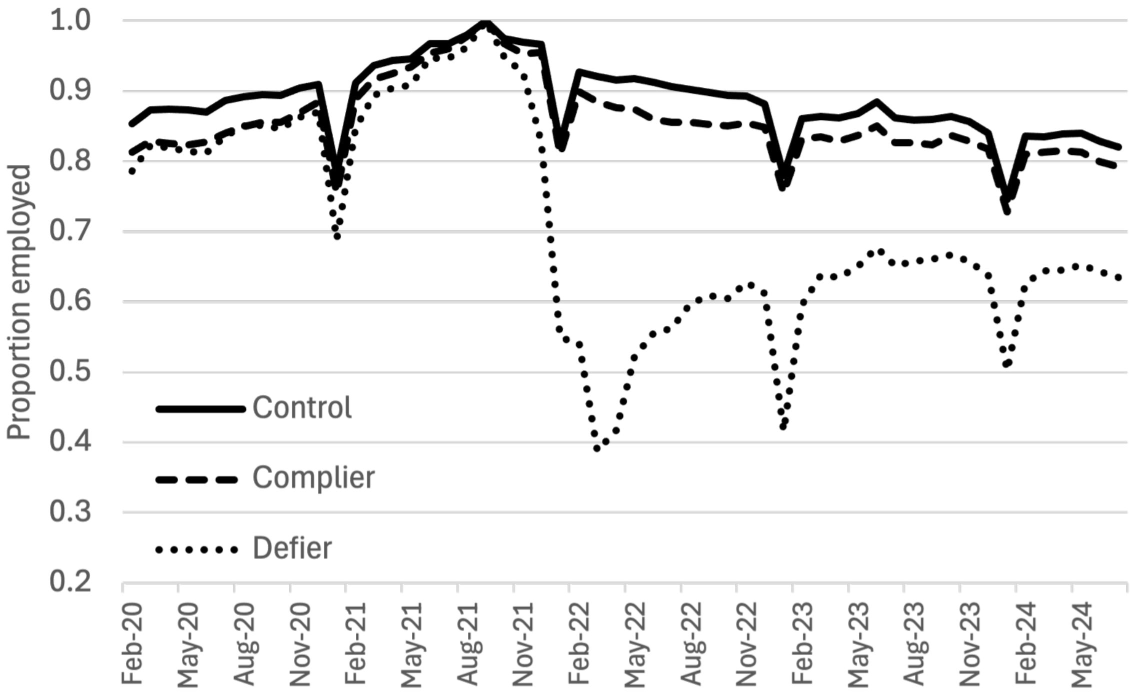
Figure 2: Covid-19 vaccination rate for health sector workers



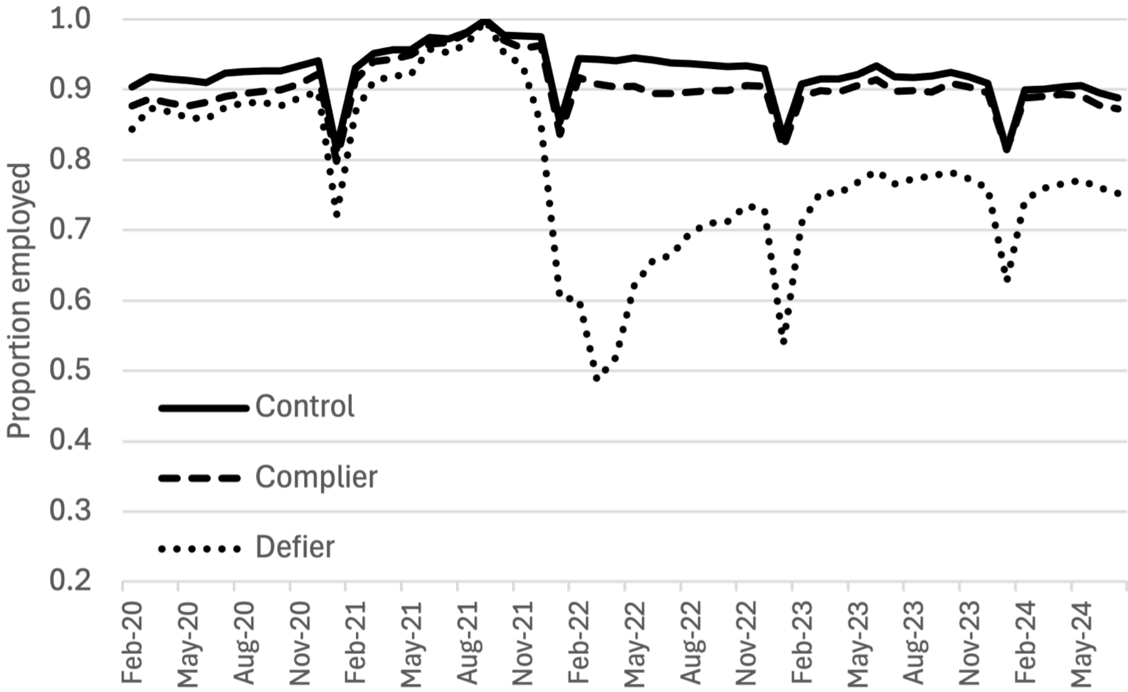
Panel (C) time period starts four months later than other panels. Vaccination rates as proportion of analysis population (ie, no adjustment for outward migration or death).

Figure 3: Employment rate for education sector workers

(A) Education sector job



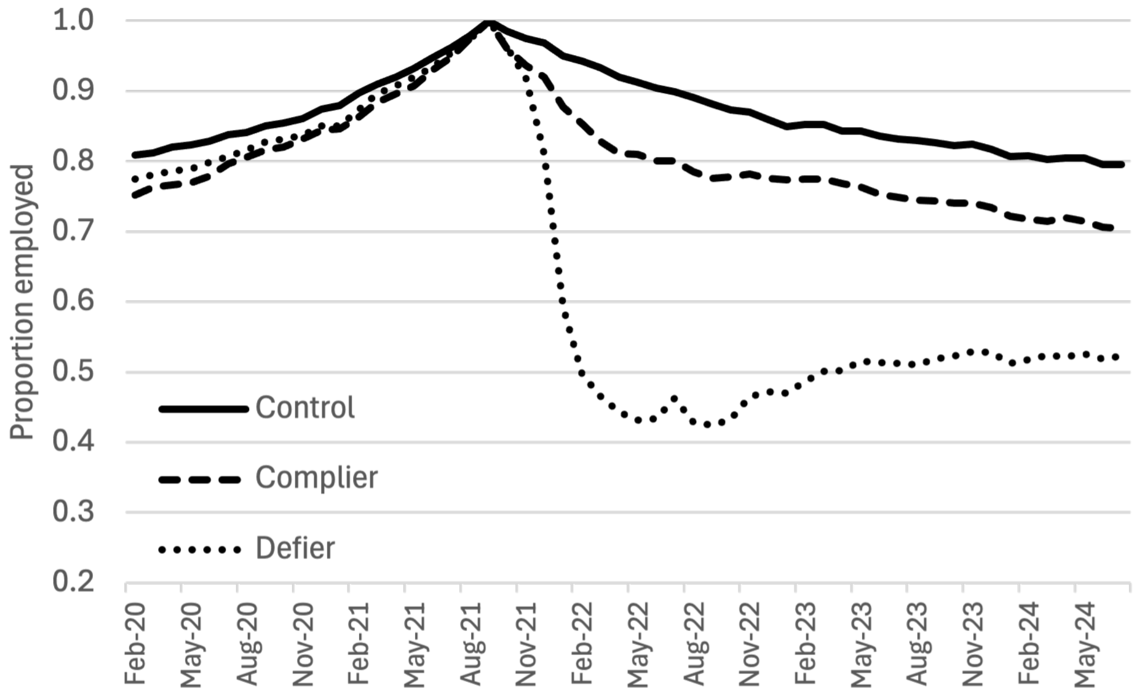
(B) Any job



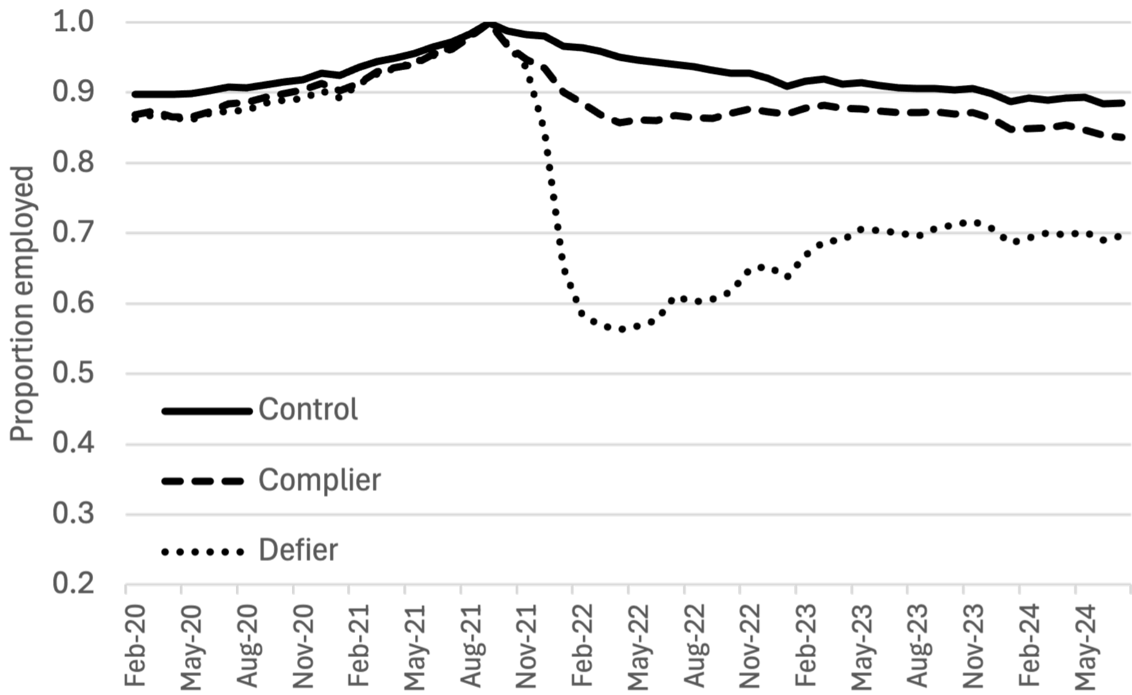
Employment rates are conditional on alive and in New Zealand.

Figure 4: Employment rate for health sector workers

(A) Health sector job



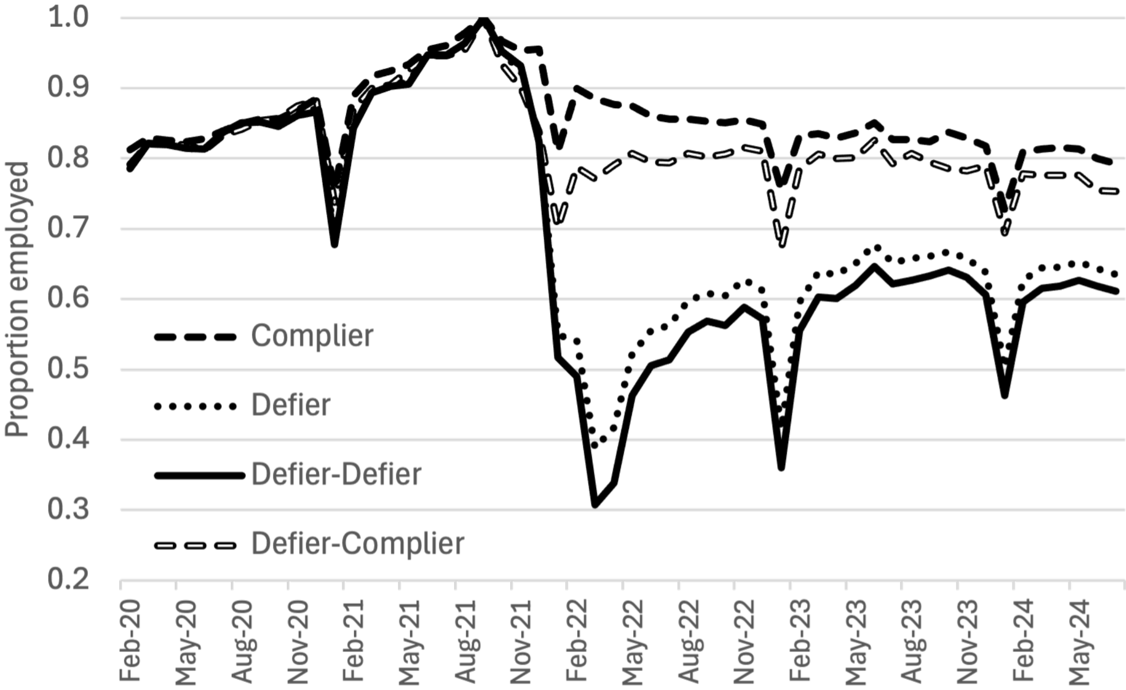
(B) Any job



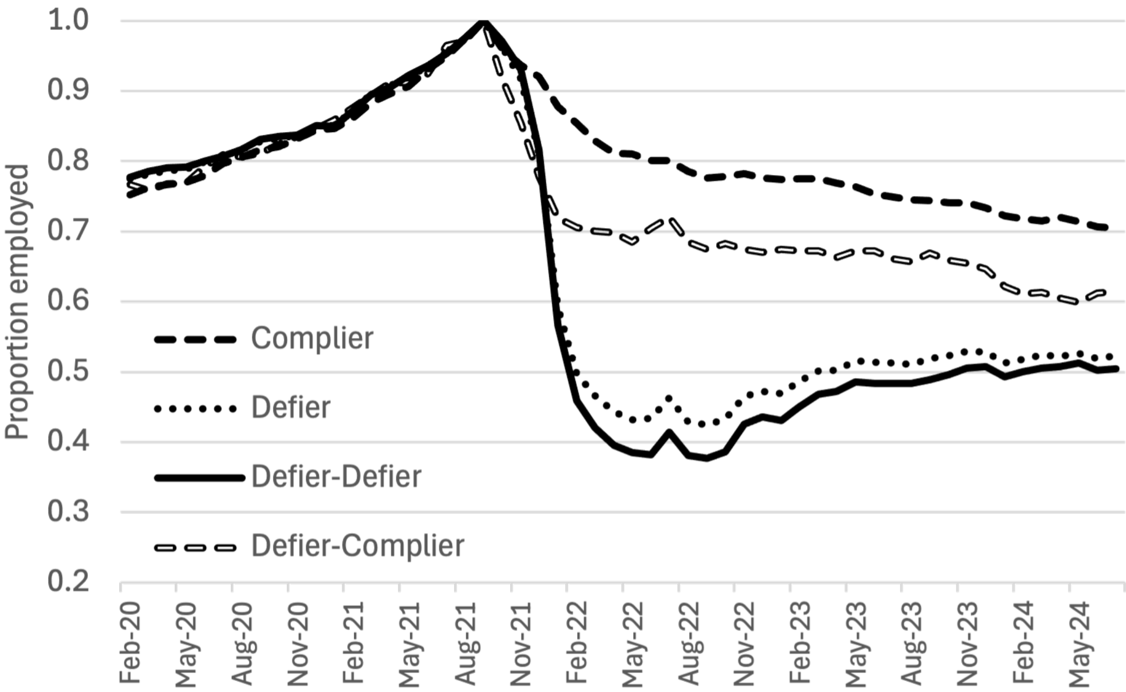
Employment rates are conditional on alive and in New Zealand.

Figure 5: Within-sector employment rate for defier subgroups

(A) Education sector

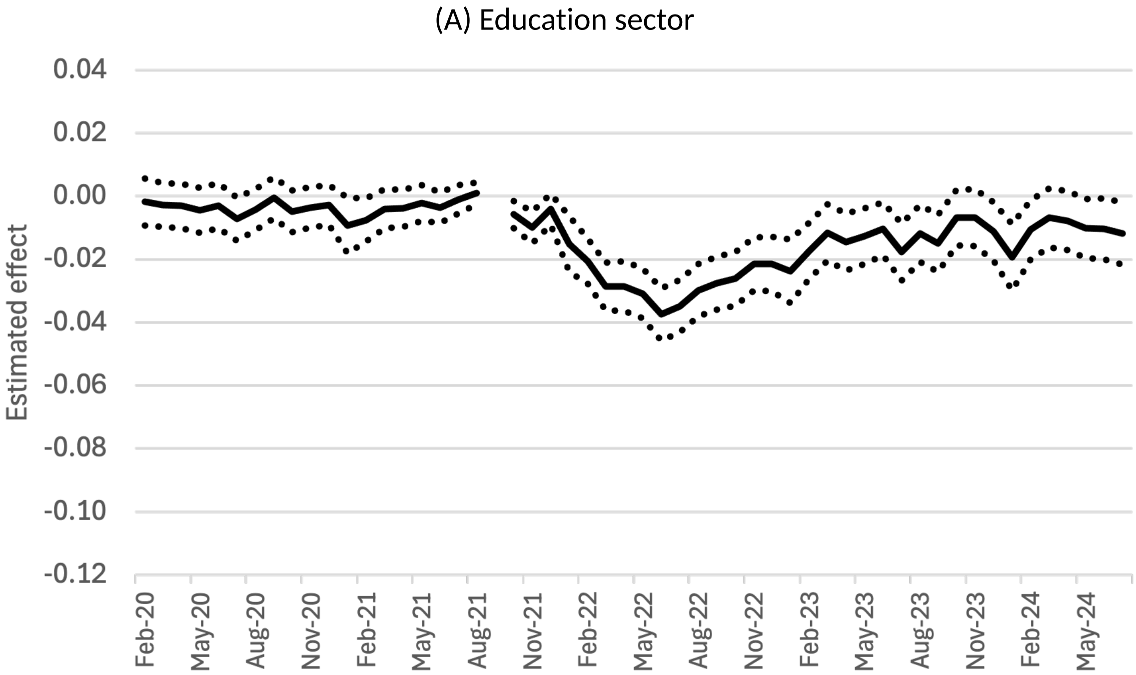


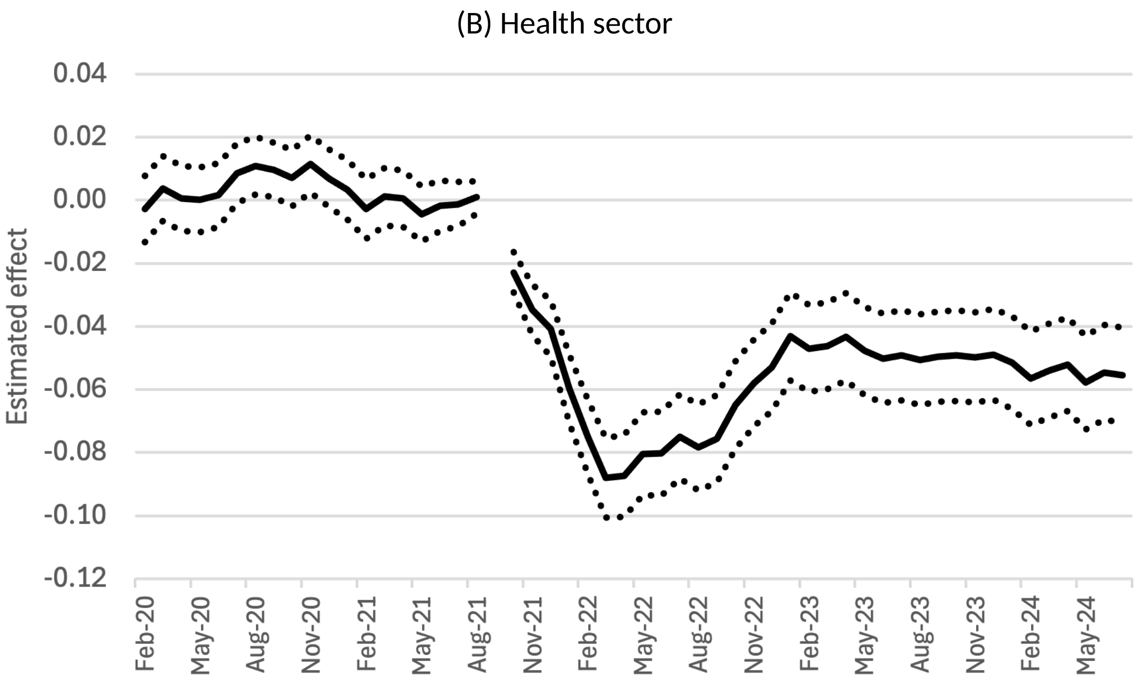
(B) Health sector



Employment rates are conditional on alive and in New Zealand. Education sector has 723 defier-compliers (17% of defiers). Health sector has 516 defier-compliers (16% of defiers). Dashed (complier) and dotted (defier) lines repeat results from figures 3(A) and 4(A).

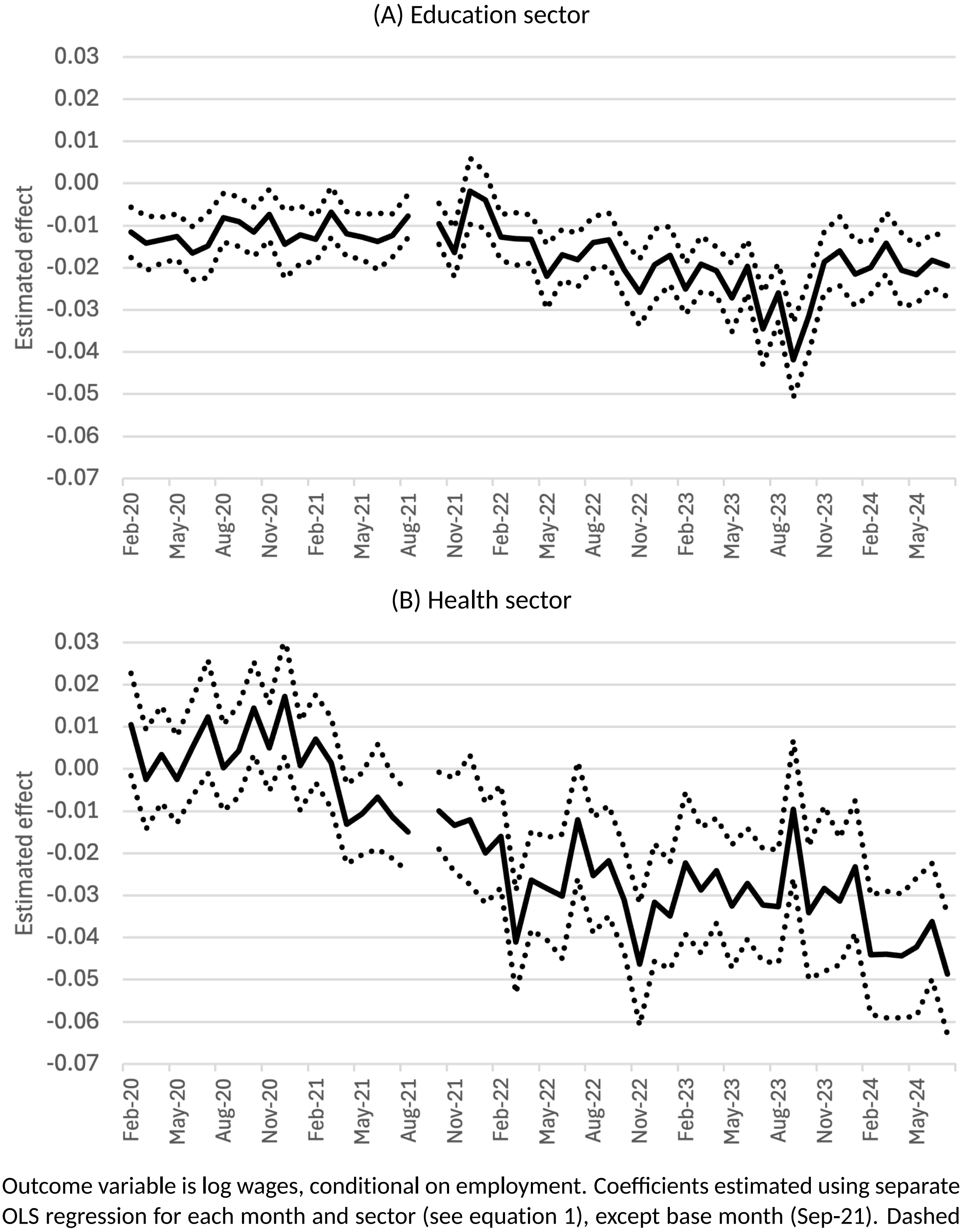
Figure 6: Estimated effect on within-sector employment for compliers





Outcome variable is within-sector employment, conditional on alive and in New Zealand. Coefficients estimated using separate OLS regression for each month and sector (see equation 1), except base month (Sep-21). Dashed lines show 95% confidence interval (robust standard errors). See table 4 note for additional covariates.

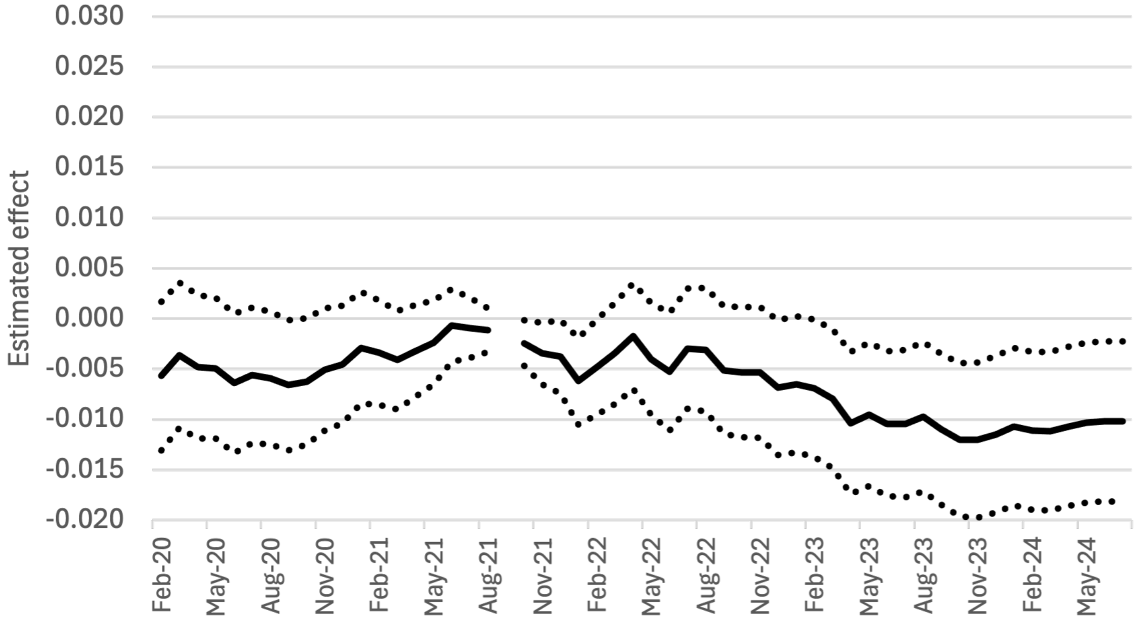
Figure 7: Estimated effect on log wages for compliers



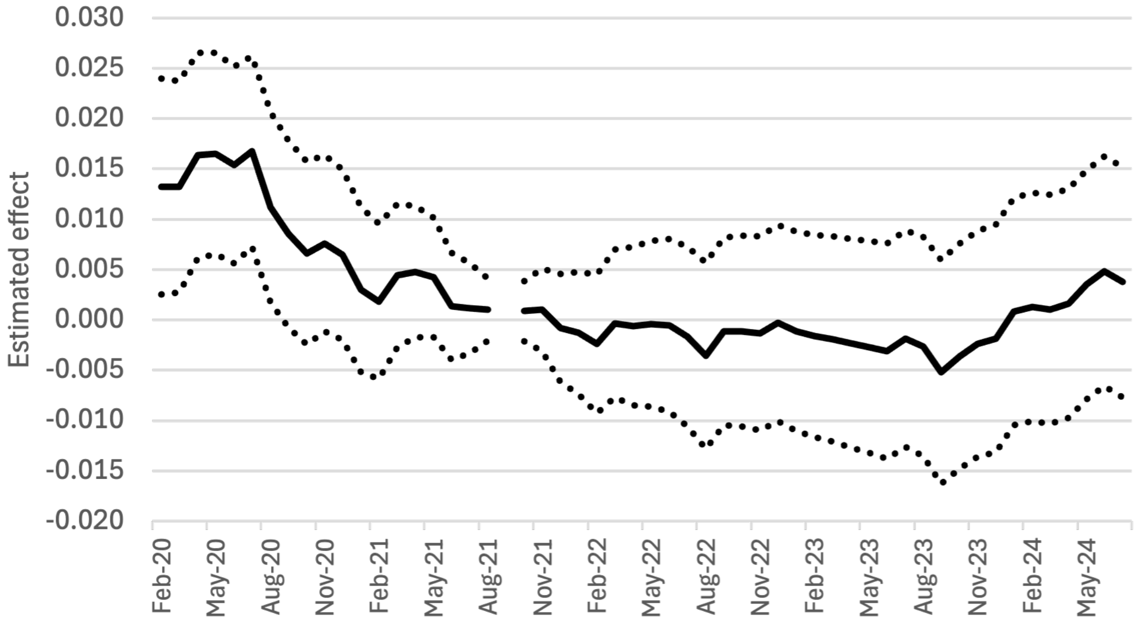
Outcome variable is log wages, conditional on employment. Coefficients estimated using separate OLS regression for each month and sector (see equation 1), except base month (Sep-21). Dashed lines show 95% confidence interval (robust standard errors). See table 4 note for additional covariates.

Figure 8: Estimated effect on stable residential location for compliers

(A) Education sector

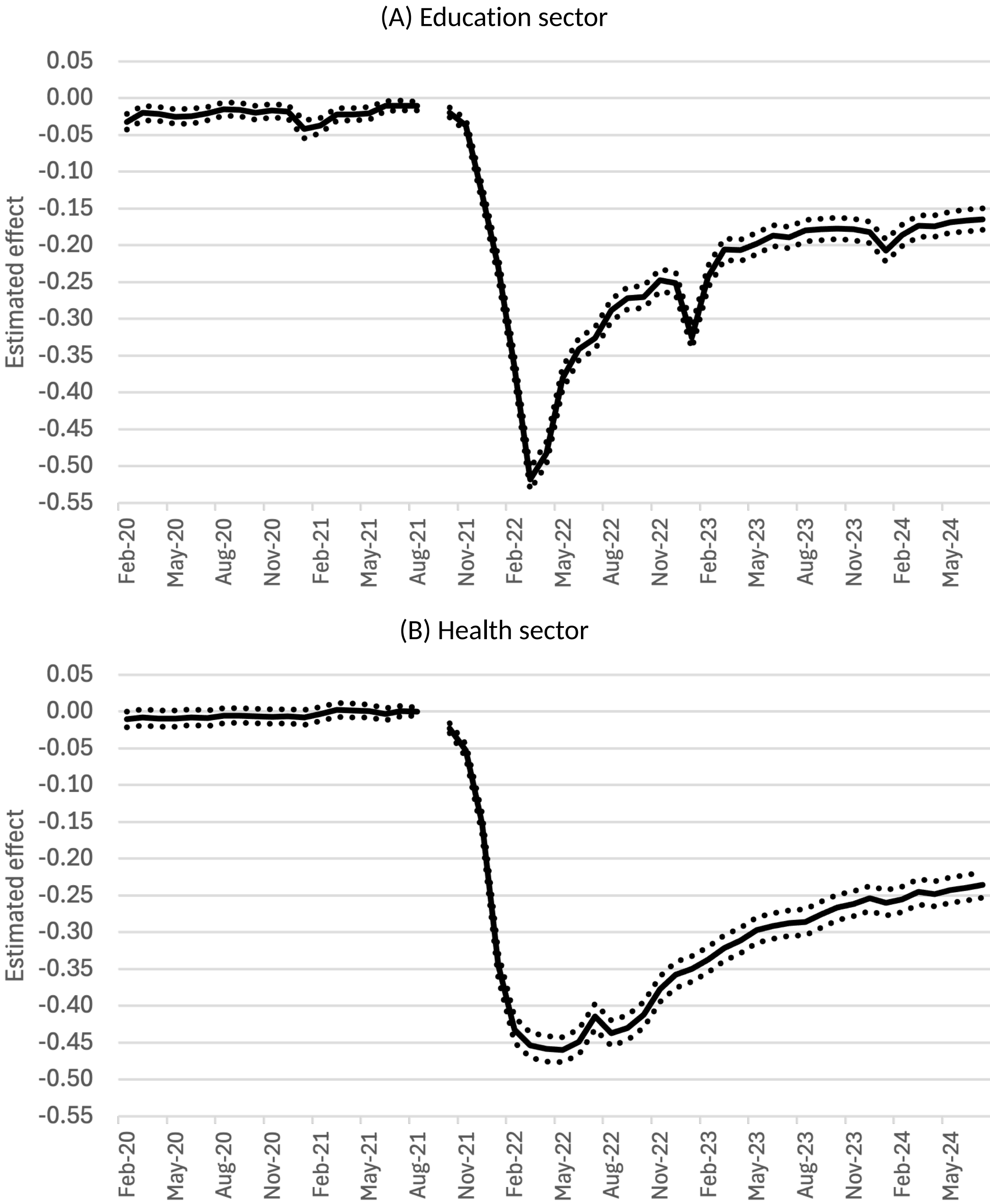


(B) Health sector



Outcome variable is living in the base month Functional Urban Area (FUA), conditional on alive and in New Zealand. Coefficients estimated using separate OLS regression for each month and sector (see equation 1), except base month (Sep-21). Dashed lines show 95% confidence interval (robust standard errors). See table 4 note for additional covariates.

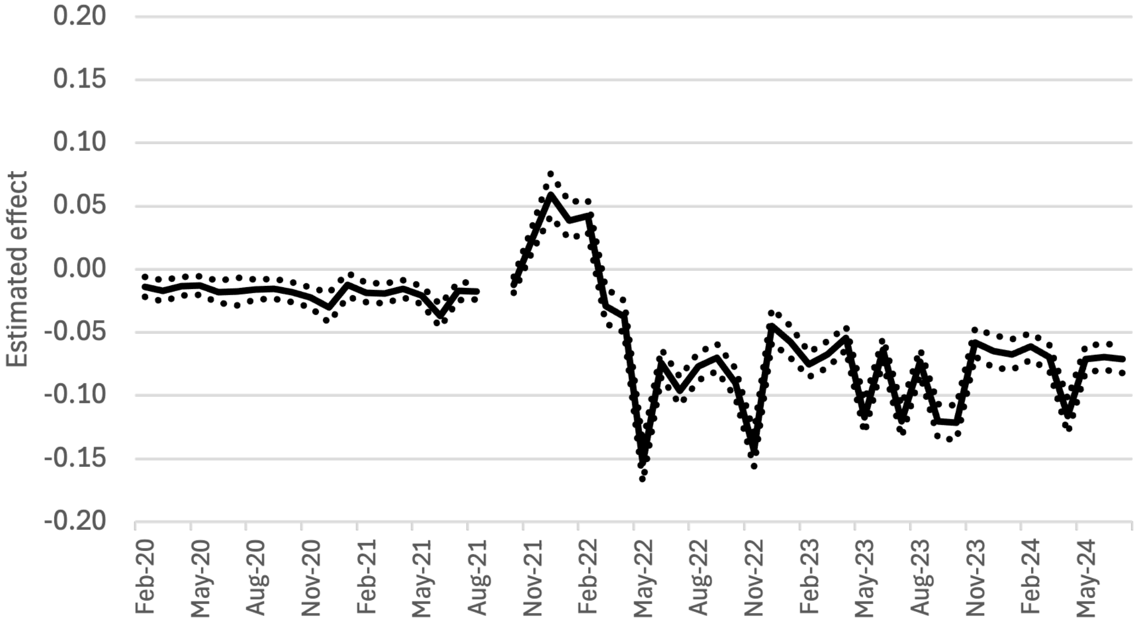
Figure 9: Estimated effect on within-sector employment for defiers



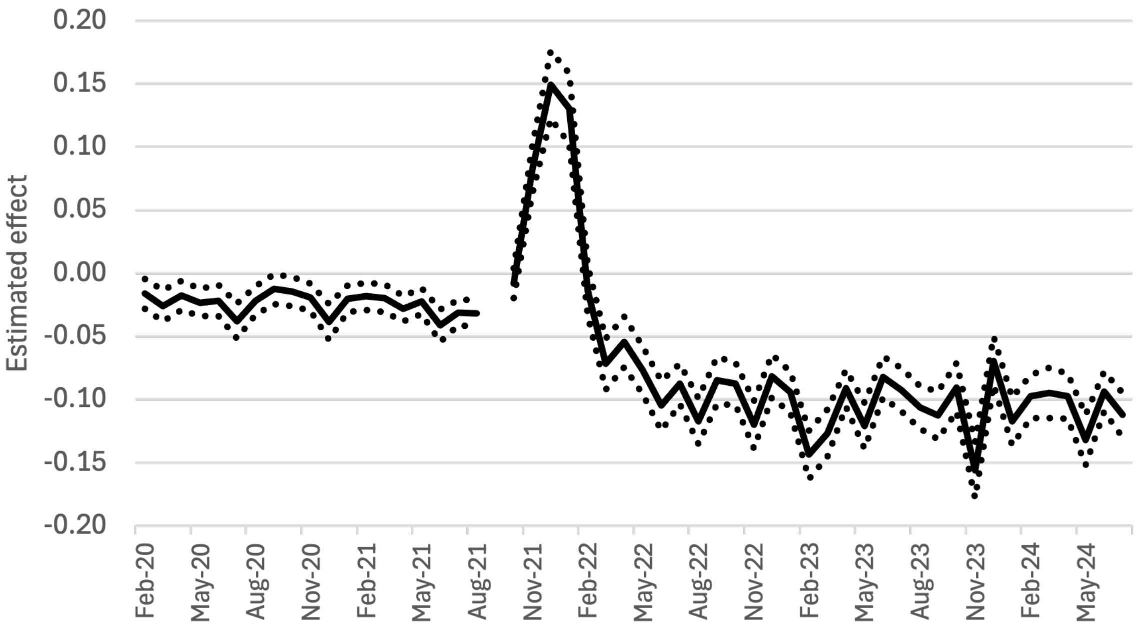
Outcome variable is within-sector employment, conditional on alive and in New Zealand. Coefficients estimated using separate OLS regression for each month and sector (see equation 1), except base month (Sep-21). Dashed lines show 95% confidence interval (robust standard errors). See table 4 note for additional covariates.

Figure 10: Estimated effect on log wages for defiers

(A) Education sector

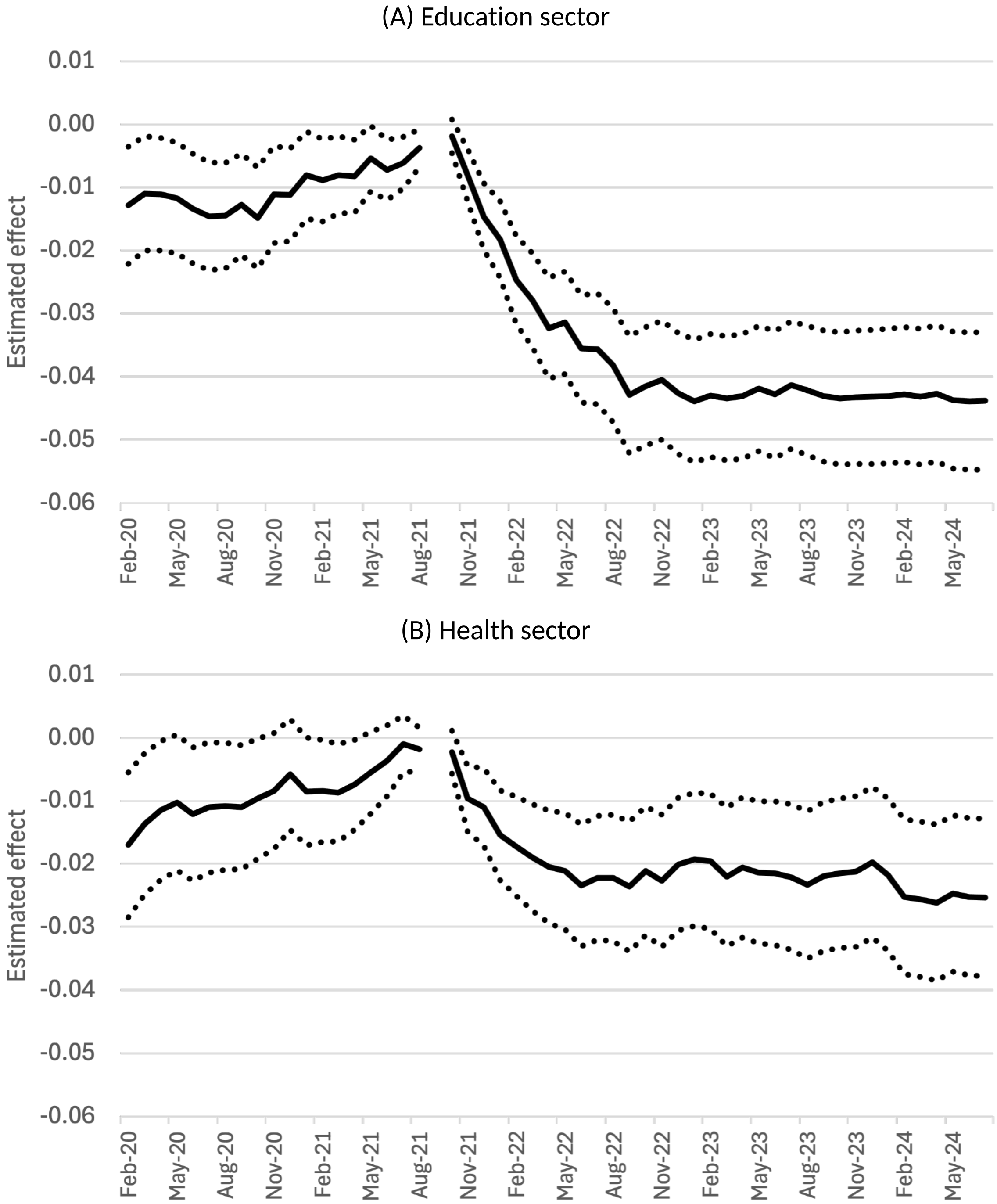


(B) Health sector



Outcome variable is log wages, conditional on employment. Coefficients estimated using separate OLS regression for each month and sector (see equation 1), except base month (Sep-21). Dashed lines show 95% confidence interval (robust standard errors). See table 4 note for additional covariates.

Figure 11: Estimated effect on stable residential location for defiers



Outcome variable is living in the base month Functional Urban Area (FUA), conditional on alive and in New Zealand. Coefficients estimated using separate OLS regression for each month and sector (see equation 1), except base month (Sep-21). Dashed lines show 95% confidence interval (robust standard errors). See table 4 note for additional covariates.

A. Appendix – Critique of Meehan, Mitchell, and Pacheco (2024)

Prior analysis of vaccine mandates impacts on New Zealand workers has limited utility as a comparator for the current study because:

- The worker population is determined two years prior to mandating, meaning that the majority of “unvaccinated health care workers” aren’t in the sector at the time of mandating (their figure 5). A comparable number of health care workers will join the sector during this period but are excluded from the analysis. These joiners are skewed towards younger workers, who are more likely to be affected by the mandate

- The worker population excludes workers who leave New Zealand during the analysis period, but this is a potential mandate effect, meaning the restriction may remove workers who had initially bad labour market outcomes but were initially constrained from leaving New Zealand because border movement was restricted

- Parallel trend violations are not explored, but plausibly arise from comparing jobs with substantial occupational requirements (health, education) with jobs that don’t have such requirements (eg, retail, hospitality). The authors use triple difference-in-difference methods, but key results still display pre-trends (eg, their figure 7B)

- Estimated mandate uptake effects are nonsensical (ie, negative), which the authors acknowledge but also conclude that “...workforce vaccine mandates provided limited benefit in terms of increasing vaccination rates among mandated workers.” (their abstract). Aside from self-contradictory, such results cast doubt on subsequent analysis using the same data

- Employment effects are restricted to the health sector, despite the relatively lower early vaccination rate in the education sector strongly suggesting that the impact of mandates on that sector could be large

- The (limited) education analysis excludes a significant proportion of early childhood education (ECE) which, though classified to a different industry division (Q8710), has a similar proportion of ECE teachers as preschool education (P8010) with an identifiable proportion of firms switching between classifications

- The authors use their recurring set of covariates in regressions ignoring key covariates, such as employment history, which are highly pertinent and easy to construct[13]

[13] Their covariates include “prioritised ethnicity,” which is an outdated approach and should no longer be attributed to Stats NZ.

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